bacteria to lose their power of producing pigment, either temporarily, as in the case of the *bacillus prodigiosus*, or even permanently, in the case of the *bacillus ruber* of Kiel. We are, therefore, surprised at being categorically informed, both in the introduction and in the appendix of this work, that pigment is formed especially under the influence of light, a statement which is entirely out of harmony with the observations of Laurent, and for which the experimental foundation should have been carefully set forth.

These and other points of a similar character will doubtless be rectified by the translator in preparing a second edition, which it would be well to amplify with references to literature, with which even an elementary student in a new science must at once be made familiar. The illustrations are in the majority of cases very good, and contrast most favourably with those we have seen in some recent works of the kind in which photographic representations have been attempted. The coloured prints of cholera and typhoid bacilli are especially excellent.

OUR BOOK SHELF.

Exploration of Mount Kina Balu, North Borneo. By John Whitehead. (London: Gurney and Jackson, 1893.)

MR. JOHN WHITEHEAD belongs to the much-maligned class of field-naturalists. For the purpose of obtaining a knowledge of the ornithology of Mount Kina Balu, he spent nearly four years collecting in the region, and accumulated a large number of new species. In addition to visiting North Borneo, he stayed some time at Java and Palawan, and made an expedition into the State of Malacca. The rather cumbersome volume before us recounts the story of these explorations. It consists of 192 pages of general description and 115 pages of matter reprinted from the proceedings of various Societies. Thirty-two excellent plates illustrate specimens from the extensive zoological collections made by Mr. Whitehead, and the places and peoples seen by him. It need hardly be said that these add considerably to the value of the book. Several woodcuts are also included. It would be ungracious to find fault with Mr. Whitehead for looseness of expression, since he craves indulgence for his "literary shortcomings." He found it far easier to explore an unknown tract of country than to write an account of his travels. Like some other travellers who have given to the world accounts of their wanderings, Mr. Whitehead dwells too much on trivialities. But for all that, there is much that is new and interesting in the book, and one cannot but admire the indomitable spirit which carried the author through numerous difficulties, and enabled him at last to reach an altitude of 13,525 feet on the mountain of Kina Balu.

Pillow Problems. Curiosa Mathematica, Part II. By Charles L. Dodgson, M.A. (London: Macmillan and Co., 1893.)

IN these pages we have a series of problems worked out, or, as the author says, "nearly all thought out during sleepless nights." In the preface he informs us the exact method of procedure, and the way in which he obtained his results. The problems are about seventy in number, and deal with many branches of mathematics, but chiefly with algebra, plane geometry, and trigonometry. The order of the three and only chapters is as follows: questions, answers, and solutions; and he explains the reason for this peculiarity in the preface. Considering the problems themselves, one is apt to think that some of them at least are not so very hard, but the publication of them will be found very interesting and perhaps useful to those of ordinary mathematical powers, who may like to follow the same routine way of thinking as that adopted by the author.

The A B C Five-Figure Logarithms. By C. J. Woodward, B.Sc. (London: E. and F. N. Spon, 1893.)

THIS small book of logarithms may be said to be a second edition of the tables previously published by the author. In addition to the tables of mantissæ of numbers, the same A B C system has been applied to logarithms of arc functions, with only a slight difference in the method. Besides these the square roots of numbers (from 1 to 100) to three places of decimals are given, and a table of "numbers often wanted," and of the densities of gases, weights and measures, &c. To facilitate the finding of the logarithms, &c., a lateral index is adopted. Besides being a compact and convenient set of tables, the worker will find them easy to use, and accurate enough for such calculations as are generally met with in the physical laboratory, the class-room, &c.

Enunciations in Arithmetic, Algebra, Euclid and Trigonometry. By P. A. Thomas, B.A. (London: Macmillan and Co., 1893.)

IN these pages one is treated to a selection of some of the chief questions that relate to Arithmetic, Algebra, Euclid, and Trigonometry. Stress is laid on the more elementary parts of each subject, and several typical problems are inserted. The latter relate chiefly to the arithmetical and algebraical sections, while the Euclid section is accompanied by important riders. The book should prove acceptable to those revising these subjects, whether for examination or not, and will be, both for teachers and taught, a useful companion to the text-books in use.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Thoughts on the Bifurcation of the Sciences suggested by the Nottingham Meeting of the British Association.

THE opening paragraph of the President's address contains this sentence: "We have come to learn what progress has been made in departments of knowledge which lie outside of our own special scientific interests and occupations, to widen our views, and to correct whatever misconceptions may have arisen from the necessity which limits each of us to his own field of study."

A most worthy and attractive ideal. Something of this kind of intersectional information does go on at these meetings; but to how small an extent! It may be said, indeed, that except for the presidential address and the two evening lectures, everyone sticks to his own section, and discusses matters lying in his own groove.

This state of things is perhaps inevitable, but it is none the less to be regretted. It is extremely difficult for anyone actively engaged in the work of any one section to attempt to attend any other. I myself used to make the attempt, but concluded that the results were too precarious and uncertain to be worth the dissipation of energy involved, and have now abandoned it. Yet there can be little doubt that if the state of things postulated by the President were feasible in practice it would be a distinct gain.

But it would seem as if the modern tendency were all in the other direction. Papers in the two great scientific departments are read as far as possible on different days at the Royal Society, and are published in separate volumes. Such an arrangement is decidedly convenient: I am not repining at it. The

Royal Society type of paper is almost necessarily unintelligible to any but specialists, perhaps sometimes even to them.

But the arrangement should not be regarded as anything but a lamentable necessity. If a more conjoint character could by any device be given to the meetings of the B.A. it would be an excellent thing: so it seems to me.

Whether the British Association can or cannot act as a connecting link between the sciences, there is no doubt but that the pages of NATURE do so act; and long may it be before NATURE (I mean the publication) finds herself also bifurcated or otherwise subdivided, and we on either side cease to hear even an echo of what the other side is talking about.

Perhaps few are able to say that they read NATURE all through as Mr. Darwin did, but we all have the chance of doing so; and I hope it is the practice of the biological side to communicate to its pages at least an epitome or a popular account of all the researches which have a wide embracing interest.

Much that the chemist does—still more that the biologist does—we of the physical camp do not care to hear; partly because we might not understand it, chiefly because the research lies so far from the field we are at present occupied in cultivating, that we can perceive none of the connecting links.

But some of the problems on which the biologist—especially perhaps the physiologist—is engaged are, or might easily become, of supreme interest to physicists; notably everything connected with sense organs and the "mechanism" of sensation.

The fear is lest we drift apart so far that we cease to understand each other's language.

The current language of physics consists mainly of adaptations of simple English phrases. It is full of common words, redefined and made definite in connotation. We do indeed use the word "coefficient" occasionally, but we are getting ashamed of its length and high-falutin' character. We got it from the Mathematicians. We have also a few other long words, as Electricity and its derivatives, which we sometimes try to abbreviate without much success. We got them in the Middle Ages. But the words we coin now are as nearly monosyllabic as possible, and as near akin to the ordinary usage of language as may be.

The current language of biology is quite different. Its sentences, as exemplified in parts of the presidential address, are highly dignified and elaborate structures, not wholly different from a once more prevalent German model. Its words, especially its new words, are hendecasyllabic or, at any rate, polysyllabic. They are extremely classical, and as unlike the language of daily life as can be contrived. This is done of good set purpose, viz. to keep free from the misunderstandings arising out of the attempt to give to popular words a scientific, *i.e.* an accurate, meaning.

I suppose it is inevitable, and no doubt biologists know what is best for their own science; I only lament it because it seems likely to retard that free intercommunication between the sciences which many of us would like to see made more possible than at present it is. I shall have the President with me here; but may I put a question to him without profanity? May I ask him if he can imagine a biologist asking about a process, "What is the go of it?" I conjecture, but perhaps I am wrong, that if a young biologist wanted to know more about a most important and interesting process occurring in the blood, he would ask, "What mechanism do you consider it was which supplied the chemiotactic stimulus impelling these leucocytes towards the morbific microbes which they are devitalising?"

I do not complain of this language—very likely it is well suited for home consumption—but it does seem to render intercommunion difficult.

Now, for instance, I am anxious to learn the most recent view of biologists on the subject of life, or vitality, or vitalism, or whatever word conveys the hypothesis that the life of an organism is something different from the chemical and mechanical activities of its tissues. I see that the President touches on this very subject, but somehow I cannot seize concerning it any distinct idea, though I rejoice to see his warning against the misuse of the terms "mechanism" and "mechanical." The term "mechanical" is regarded by physicists as the *ne plus ultra* of explanation, and it is unlikely that any explanation of physiological processes will skip the intervening chemical and physical stages, and land itself straight in simple mechanics.

But I wonder if I am right in supposing that the definition of life, given apparently by Treviranus, satisfies the modern biologist, viz. (I put it only in paraphrase, for more exact wording see NATURE, September 14, p. 464), "that property of an organism which enables it to respond similarly to a variety of different stimuli"; because a steam engine or any other prime mover can do as much as that. It matters nothing by what means the throttle valve is opened, whether by the proper driver, or by a larky boy, or by a piece of string, or a falling weight, or an electric current; the result is the same—wheels go round.

This property of responding to stimuli, and responding always in the same way if at all, may be characteristic of a clock-work mouse, but surely it is not a special peculiarity of *life*. If a muscle can only twitch, then, however you tickle it, it must either twitch or do nothing.

But life surely is something other than a power of response to a stimulus; it is more like a something which directs the stimulus, more like the driver who decides whether the throttle valve shall be opened or not. But it is absurd for me to attempt to answer such questions. I only want to ask them: all I clearly perceive from the physical standpoint is that live creatures have the power of directing energy into otherwise unoccupied channels, and that life in itself, whatever it is, is *not* a form of energy.

But this leads me to a subject which though apparently trivial may, if not attended to, have serious or at any rate inconvenient consequences, I mean the occasional misuse by one science of the language of another science.

The term "energy" is a physical one given us by Thos. Young, and it has been fought for by us through a great part of this century. It will be wanted seriously by Physiologists before long, in its proper sense, and it will be a thousand pities if they misuse it.

If it be urged, "but Helmholtz used the term *specific energies*," I reply yes, a long time ago, and so also he used the phrase, *Erhaltung der Kraft*. But precision in the use of the term energy is of comparatively modern growth, and every one now translates *Erhaltung der Kraft* as "conservation of energy." So, also, I venture to think, they should usually translate the phrase "specific energy" by the words *normal activity* or *normal reaction*. Of course if normal activity of an organ or tissue does not represent the thing meant, that is another matter—so far as I can judge, it usually does; but whether it does or not, I am clear that specific energy is usually wrong. There is one definite theory or hypothesis, to express which the words energy in some form would be correct—viz. when it is meant to assert that, for instance when light falls upon the retina, all it does is to pull a trigger, and the explosion or nerve stimulus which results is due to energy in or near the nerve ending itself. If that is a true statement of the case, and there must be a great deal to be said for such a view, the latent energy of the organ can no doubt be measured. But inasmuch as energy is all one thing in many forms, the adjective *specific* is better omitted; moreover the phrase is not usually limited to this particular hypothesis; and by "the specific energy of an organ," is usually meant, not anything quantitative, but simply the mode in which it normally reacts.

Another case of terminology occurs to me. For specification of small lengths microscopists have introduced the term *micron* for a thousandth of a millimetre, or a millionth of a metre; and very handy is both the magnitude and the name, and I hope physicists will adopt it. But everyone should consent to use it in the same sense. There was a discussion about it in the pages of NATURE a few years ago, but I am not sure that the usage even now is quite distinct. Many biologists call it a micro-millimetre, which it is not; and though they may mean the same thing, it can only be by an erroneous, because unconventional, use of the prefix micro. All these things are conventions, and once made the convention should be rigorously adhered to. Sometimes the word is written *micro* instead of micron; a very small divergence, but better avoided. Either term will do perfectly well, but not both.

May we understand then that a micron is a micro-metre, or a milli-milli-metre, or 10^{-4} centimetre; and that a millimicron is a micro-millimetre, or 10^{-7} centim.?

And may I incidentally protest against too much public use of the meaningless and wasteful symbols μ and $\mu\mu$ for these two lengths. If these symbols are found too handy in technical microscopy to be abandoned, they must be used there; but they should never be allowed to obtrude into anything intended for the general reader, nor for workers in other departments of science.

I trust that physicists will agree with me in this. I know that some Electricians try to sin in a similar way, by writing 6ω when they mean 6 ohms. But with all deference to any individuals who may have allowed themselves carelessly to drift into this practice, it is a thoroughly bad precedent. We shall soon be having $12a$ and $5v$ and $\cdot 3h$ for current and voltage and inductance respectively; a simple specification will look like algebra, and algebra will look like gibberish.

Similarly the custom of writing M for a millionth of an atmosphere, or 1 barad, is a worrying custom. Let us always have names for units with which we have much to do, but never single letters. Single letters have to serve a far more important purpose, that of denoting the quantities themselves—the whole of a quantity, numerical part, unit, and all.

This last is an old hobby of mine. Ever since my brother showed me the advantage of consciously interpreting algebraical symbols as standing for concrete quantities, and not merely for abstract numbers, the advantage of doing so has presented itself to me with cumulative force. Most physicists are, I think, now of a similar opinion, if they have thought at all about the matter, and Prof. Greenhill is being left almost alone in his state of grievous error; I would say heresy, but that I fear he has some of the pure mathematicians with him for company.

I have dragged Prof. Greenhill in because I want to deny the extraordinary assertion which he makes in an article on page 457 of your issue for September 14, viz. that I would like to "banish the word *hundredweight* from our language." On the contrary, for the specification of loads I have always found it a very convenient word; and if architects use it thus, for pressure on foundations, so much the better. I know what he is referring to—a part of my book on mechanics where I am instructing youth in the meaning of the term *mass*, and the difference between mass and weight. Till they are clear on this point I say that "hundredweight" is a term better avoided for the present. I should, for instance, recommend its avoidance for the present by Prof. Greenhill.

But to return to Dr. Burdon Sanderson's address, which it is perhaps evident from a former part of this article that I have been trying to read, there are two small points on which I would ask a question. First, with regard to totally colour-blind vision. If a person sees all the world in shades of gray he may properly be called colour-blind, in one, and that the most important, sense; but it does not seem to me to follow that he necessarily appreciates white, still less that he proves a specific white sense in normal eyes. On the orthodox theory, as held by physicists, such an eye would strictly be called monochromatic; one only of the three colours would be seen, and which it was would matter nothing to the seer, though it might be ascertained by studying his spectrum vision which the one colour was in any given case. I believe that Abney and Festing found it usually blue. But as regards the psychological impression produced by mono-chromatic vision on the seer, its indiscriminating monotony would obviously result in total absence of colour perception. One colour sensation is psychologically the same as none.

The other question is whether it is useful to distinguish between "physical light" and "physiological or subjective light." The term *light* applies to the stimulus as far as the retina, but after that is it not better called either *sight* or some other and more impressive-looking word, beginning with *photo* or *neuro* and perhaps ending with *taxis*, signifying the specific disturbance of the optic nerve and brain centres. These terms light, heat, sound, &c., have always been ambiguous; but, if needful to discriminate, they had better perhaps now be handed over entirely to physics, to signify monosyllabically the external physical stimulus; while fresh words are coined for the physiological, and again, where not already existing, for the psychological, result.

I trust that this letter has not the appearance of undue presumption; the whole of it is written in the key of interrogation.

Oliver J. Lodge.

British Association: Sectional Procedure.

Members of the British Association often entertain schemes for the improvement of sectional procedure, which rarely, so far as I have seen, commend themselves to the good opinion of the organising committees. I beg leave to produce one scheme more. Whether the remedy is practicable or not, I am quite sure that the grievance I have to point out is a real one.

Every member of the Association has suffered from the great uncertainty as to the hour at which a particular paper will come on. At the recent Nottingham meeting I was unlucky enough to spend one morning to no purpose. I had a direct interest in two communications; one was not reached that day, the other was taken as read. There is no care taken to prevent such accidents, and yet it would have been easy to provide against the second one at least by marking the communication as "Title only." The other case is of greater, but not, I think, of insuperable difficulty. The remedy which occurs to me is this: a fixed time should be assigned to communications which in the opinion of the Sectional Committee are of special interest and importance. There might be at least two absolute fixtures in each day's proceedings, when members would know that nothing would be allowed to interfere with the punctual production of certain papers or addresses. I should be inclined to mark these by some distinctive title, such as "Address by request of the Section." It seems to me very desirable to send out special invitations before the meeting to persons who could communicate interesting results, and I have little doubt that a fixed time would often lead to acceptance by persons whom the Sections would be glad to hear, but who rarely or never appear in the programme under the existing system. What is bad for the audience is bad for authors too, and after an author finds that his communication is addressed only to people who come to hear something else, and to people who in their despair are working through the entire list, he ceases to offer himself.

If the facilities granted to pre-arranged addresses should lead to a stricter treatment of trivial papers and business matter of no direct scientific interest, the Sections would not suffer.

L. C. M.

Orientation of Temples by the Pleiades.

Eighteen months ago, while at the Mena House, Cairo, I came across a back number of Nature, which contained an article on "The Origin of the Year," in which reference is made to the orientation of some Egyptian temples, and I suggested that inquiries should be made as to whether they were not in some cases oriented by the Pleiades. I had not then seen the numbers that referred to stellar orientation.

A pamphlet of 105 pp. was privately printed by myself *thirty years ago* (!) for my own use in the prosecution of "A Comparison of the Calendars and Festivals of Nations," with special reference to the Pleiades.

Since that pamphlet, and a second, of about 20 pp. on cycles regulated by the Pleiades, were printed, I have collected a great deal of further data confirming the conclusions arrived at in 1863. Müller says, in his Religion, &c., of the Dorians, I. 337, that the famous eighth-year cycle, which was in general use in Greece, was luni-sidereal, and regulated by the Pleiades, and that the great feasts of Apollo at Delphi, Crete, and Thebes, were arranged by it. He also states (p. 338) that there are vestiges of a sacred calendar in general use in Greece in early ages based on this cycle, but that it fell into disuse, and, in consequence, the Attic festivals and months were thrown into confusion. He had previously stated that the Olympiads were based on the eight-year cycle. Apollo, generally assumed to have been essentially a solar deity, though he evidently was originally a type of Karlikeya, was a god of the Pleiades, and hence the seventh day was sacred to him at Athens. As those stars were the daughters of Atlas, the forty days during which they deserted the nightly sky were spent by Apollo in dancing and singing among the Hyperboreans of Marlas. When the rising of the Pleiades at early morning took place, he returned. In 1882, at the American Association, I showed that he is still remembered south of the Atlas as "Apōlo, a good god, who comes and plays upon the harp." But in the lapse of centuries the Pleiades seemed to go astray, and were forgotten, and, strange to say, Athenæus was forced to treat the history of the Pleiades as a bit of obsolete folk-lore. In discussing the subject of the two groups of *Peleiades* on the handles of the divining cup of Nestor, he says that it is a mistake to suppose that Homer by *Peleiades* meant "doves" (a mistake which Mr. Gladstone has also made in his Homeric Studies), and he explains that the cup had two clusters of seven stars represented on it. Many persons, he says, are puzzled at the prominence thus given to those stars, but in early times they were regarded as very important, and left their impress on early mythology, and he also shows that they once regulated the time